September 29, 1952

Dear Norton:

From comments by many people, I learn that your presentation at Ithaca was most successful. I was pleased, but not surprised to hear this.

Now I have some bad news. Going into the lyophilised Salmonellas, I found that the sealing-off of the tubes was not very well done on ahmshwhile, so that very many of the tubes were lost. About 1 tube in every 4 or 5 is broken, spilling over with moisture absorbed by the Calcium Chloride. The storage envelopes are rather messy, but are being culled now. The technical defects that are responsible are: 1) fused calcium chloride near the neck, 2) glass pulled out too thinly, not having been adequately thickened, and 3) the sealed glass having been pushed directly back, rather than carefully melted down to give a stronger bead. If you propose to continue this method of preservation, these points have to be watched, which is why I mention them.

Not very many cultures are irreparably lost, just as a consequence of the Poisson distribution of breaks, but quite a number are represented by only a single tube. When time permits, I will regenerate the more important of these. Quite a few tubes have been opened already, and every one that was technically satisfactory has been grown out very nicely. I can't give you a full list of the losses, as only a couple of boxes have been examined. The most important so far are SW-36 (the Webster strain), LT-18 (#100), and SY-23. Less important also: LT-14(altern. 11412); LT-3 (al. 525); LT-9 (alts. 497 and 504). I will send you full details when we have gone over the entire collection. Meanwhile, it would be helpful if you could send a list of the set you took with you, and not throw ataanything until the damage is assessed. The situation is far from catastrophic, but it is unfortunate that our sense of security in the integrity of the collection should be impaired.

Stocker must have given you a full account of the summer's work. Esther and I certainly enjoyed his visit very much. Since I got back from vacation, I've taken up with Salmonella again. The "spontaneous" i from SW-543 was almost certainly an error. In addition to Stocker's set, I have some more since, evidently all b——. Bruce's explanation of two closely linked factors, one for flagella; the other forbthe H-antigenic type sounds very plausible, but it's a pity we still don't have another case whebe the components are individually and independeably recognizable. I am in the midst of an attempted confirmation, by throwing in FA for other flagellar types. I have what look like very satisfactory preparations of PLT-22/ on S. abony, dublin-ϕ, sandiego and Bruce set some up for para B (diphasic and phase II). The transductions have gone, and serological tests are in progress. If in each case the motile forms from SW-583 are a mixture of b and the FA type the two-locus hypothesis seems fairly secure.

Did we have some b serum, or did it all go into typing reagent? I can't find any. Also there is a bottle labelled pullorum serum (DTB), with an additional note on it "IXOabsorbed". Does the latter mean anything? One adhesive label was pasted over a paper label, the latter with your handwriting.

Joy Palm just gave me your note. If you have SW-36 (as I suppose you must) could you send it back for the vaccine, or can Schneider or yourself recommend a better one?

Larry and Esther are collaborating on the "gal-duction" story. Except for the restricted range, the stor is not essentially different from Salmonella, but I think we can do better on mechanism, as the types of response to lambda are more clear cut, and we are less likely to be confused by extraneous phages. (And of course we have sex). However, many of the "gal-ductions" are unstable, from + to -, and this moved me to look further into this in Salmonella. As I recall you had done quite an intensive study on a few isolates, but had not looked at a great many. However, 16 additional separate Gal+ in SW-435 + FA-PLT22/2 were all certainly stable. I have some more tests in the mill, but no expectation of any difference,

I tried to induce SW-543 to H+ with UV: no luck at all, in what should have been a very sensitive test system.

As you can see, my Salmonella work is all odds and ends for the moment. I am waiting for Spicer to arrive before planning a definite program. Perhaps you will see him this week. Tom Nelson is here, and moving along very nicely with kinetic experiments, etc. on Hfr. How are you progressing yourself in setting up your lab. and so forth?

Sincerely,

Joshua Lederberg